

A Sylow Factor Table of the First Twelve Thousand Numbers, giving the Possible Number of Sylow Sub-Groups of a Group of Given Order between the Limits of 0 and 12,000. By H. W. Stager. Pp. xii+120. (Washington: Carnegie Institution of Washington, 1916.) Price 4.50 dollars.

THE main object of this publication is to answer the question: Given n , the order of a group, what are the possible orders of such Sylow sub-groups as it contains? This amounts to finding all divisors of n which are of the form p^k ($k \geq 1$), where p is prime. For each n up to 11,999 the table gives the complete resolution of n into its prime factors, and the values of k (other than 0 and 2, which do not require entering) corresponding to each prime factor. Each prime value of n is entered in the body of the table in the form p_1 ; for instance, the entry p_{627} under 4639 shows that the latter is the 627th prime in order of magnitude, taking $p_1=1$. It is obvious that, apart from its special purpose, this table will be very useful to arithmeticians; every reasonable precaution seems to have been taken to make it accurate, and fortunately the table is of such a kind that every single entry can be tested with very little trouble, and any misprint almost certainly detected, unless a number n has been entered as prime, when really composite. Cases where $p^k (kp+1)=n$, and not merely a divisor of n , are noted, such numbers are called P s by the compiler—for instance, $1074=3(3 \cdot 119+1)$, so 1074 is a P . On pp. xi and xii is a list of these numbers (1–12,229) in their natural order; and there are interesting tables and graphs on the distribution of P numbers and primes. Supposing that $\phi(n)$ means the number of primes not exceeding n , and $\psi(n)$ the number of P numbers not exceeding n , the tables suggest that when $n \rightarrow \infty$ the ratio $\psi(n)/\phi(n)$ converges to a definite limit not very different from e ; of course this is a mere guess that might occur to anyone, but at any rate to find a formula for $\psi(n)$ analogous to Riemann's for $\phi(n)$ would be an interesting problem. It may not be superfluous to add that the table does not profess to enumerate *actually existent* Sylow sub-groups for different values of n .

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Aeroplanes and Atmospheric Gustiness.

THE question which Prof. McAdie raises in his letter in NATURE of April 12 is how to measure the unsteadiness of the air as it affects an aeroplane. Among those connected with flying the term "bumpiness" is used to express the unsteadiness of the air as it affects an aeroplane, and, in the absence of a better word, this may be used here. The problem is, then, to measure the "bumpiness" of the air by meteorological means.

As Prof. McAdie points out, gusts may occur in any

direction, but gusts in different directions will not equally affect an aeroplane, those in a vertical direction having the greatest effect. There is evidence to show that the fluctuation in velocity of any individual portion of air is, on the average, roughly the same in any direction, so that, in view of the greater effect of vertical gusts, the fluctuation of the wind, as given by an anemometer, may give entirely erroneous indications of the "bumpiness." The best example of this is in the middle of a hot summer day, with a light wind, and sky partly covered with small cumulus clouds (themselves an indication of vertical currents). Under these conditions, the variation shown by an anemometer may be less than three metres per second, but the air will be very "bumpy" for an aeroplane.

On sunless days, with strong winds, the air is "bumpy" on account of the eddy motion set up by friction with the surface of the ground. If the conditions be the same, the fluctuation is roughly proportional to the mean velocity of the wind, but under different conditions—e.g. between night and day—the fluctuation may be very different for the same strength of wind. When it is remembered that the mean velocity of the wind does not in itself affect an aeroplane when flying (except as regards getting from place to place), it will be seen that the mean velocity of the wind should not enter into measurements of the "bumpiness" of the air. Further, the gustiness of the air near the ground is of little importance, except in getting off and landing, and also it cannot be taken as an indication of the "bumpiness" of the air at a height.

To obtain satisfactory information, it would be necessary to measure the fluctuation of velocity in three directions and at various heights. Several methods have been devised for obtaining the vertical velocity, as well as the horizontal velocity and direction, near the ground; to obtain such data at a height is very much more difficult.

The "bumpiness" of the air might be measured by an accelerometer carried on an aeroplane; but this would not be satisfactory to the meteorologist, since it would be difficult to discriminate between vertical currents and horizontal gusts. It may be pointed out that by the use of both a gravity-controlled and a spring-controlled air-speed indicator, this is at least theoretically possible.

If we suppose a satisfactory means of measuring the "bumpiness" to have been devised and standardised, it would be possible to compare the average "bumpiness," say, at one place with that at another, from which, no doubt, some useful information could be obtained. But to make real advance in this part of meteorology, it is necessary to go further and endeavour to find the real structure of the atmosphere and the causes which may give rise to this structure. It is, therefore, to be hoped that the work of meteorologists interested in this branch of the subject may be devoted more and more to these fundamental problems of cause and effect, rather than to the making and tabulation of routine observations, other than those made with the definite object of throwing light on some particular problem.

GORDON DOBSON.

Farnborough, April 16.

IN reply to Prof. Alexander McAdie's request (NATURE, April 12) for a means of recording gustiness, I venture to throw out the suggestion that this might be done by observing what in German is called "wimmern." "Hoert Ihr's wimmern hoch vom Thurn? Das ist Sturm." This variation in the sounds heard from church bells during gusty weather is due to the irregular velocities in the atmosphere. That part of

a whirl of air which moves in the same direction as the sound will increase the pitch of the note heard, and *vice versa*. If an instrument could be devised for recording rapid but slight changes of pitch of musical notes, a fairly accurate estimate of the irregularities of atmospheric velocities could be obtained.

C. E. STROMEYER.

Lancefield, West Didsbury,
April 15.

Gravitation and Thermodynamics.

As pointed out by Sir Oliver Lodge in NATURE of April 5, the case I cited of a disc pivoted about its centre of mass and started in motion does not lead to perpetual motion in the ordinary sense. But, as it seems to me, there will be long-sustained, oscillatory, motion. On rotating the disc by a small angle, the descending half will gain, and the ascending half will lose, heat. A turning-moment will arise, and will increase until the angle turned is $\pi/2$. It will then decrease until, when the angle turned is π , the moment is nil. This position gives the condition, quoted by Sir Oliver Lodge, of thermal symmetry about the vertical diameter of the disc. As the disc continues to rotate, due to its momentum, a moment in a reverse sense will be set up, so that when the whole rotation is nearly 2π , the rotation will be reversed. We shall thus obtain an oscillatory motion, long sustained if small friction is involved. In the final position the disc will have rotated from its initial position by angle π . Such a result would be phenomenal, since the mere act of rotating the disc by an infinitesimal angle would, in effect, convert a condition of neutral, into one of unstable, equilibrium.

Dr. Todd has suggested that this peculiar effect would not arise if $\partial\theta/\partial r$ is positive. We should then have the remaining peculiar effect, viz. a condition of neutral equilibrium would by a rotation be converted into one of stable equilibrium. Will this proposal satisfy philosophers?

P. E. SHAW.

Floating Earths.

I SENT Dr. Leaf's letter on "Floating Earths" (NATURE, March 15) to M. Salomon Reinach, and append his answer, which, I think, partly explains the matter.

J. OFFORD.

94 Gloucester Road, South Kensington, S.W.7.

DEAR MR. OFFORD,—

You won't build on uncertain texts. Dr. Leaf translates "in the case of an islet in Tyrrhenia," reading *νηός*, which is corrupt; I prefer *γῆς* (Corey), a certain variety of earth in Tyrrhenia—the remainder concerning natural science, excepting *ἡ τὰ ἀργυροματὰ ἐκκρίτται*, words which I take in the "trivial meaning."—Yours truly,

S. REINACH.

Musée National, Saint-Germain,
Le 4 avril 1917.

The New Food Orders.

IN my article in NATURE of April 12 there were inadvertently omitted, probably by myself, some words which make one of my suggestions an absurdity. In reference to the meat ration of the Army, what I meant to say was that "a part of the large meat allowance might, with advantage, be replaced by its equivalent in energy-value of carbohydrate."

W. M. BAYLISS.

EMPIRE DEVELOPMENT AND ORGANISATION.

THE final report of the Royal Commission on the natural resources, trade, and legislation of certain portions of his Majesty's Dominions has recently been issued and presented to both Houses of Parliament. The commission came into being in consequence of a resolution passed by the Imperial Conference in 1911. The members were appointed in April, 1912, six representing the United Kingdom, and one each the self-governing Dominions of Canada, Australia, New Zealand, the Union of South Africa, and Newfoundland. India, the Crown Colonies, and the Protectorates were not included.

The sittings of the commission ended, as they began, in London. In the interval the commissioners made four tours: the first to New Zealand and Australia, the second to the Union of South Africa, the third to Newfoundland and eastern Canada, and the fourth to central and western Canada. They visited every capital of every State or Province in each of the Dominions, and took evidence in all the most important cities. They say:—

In the course of this period we have travelled for many tens of thousands of miles to, through, and from the self-governing Dominions of your Majesty's Empire. In every district of this vast area we have done our utmost, collectively and individually, to make ourselves acquainted with its characteristics, its history, and its aspirations, as we hope, and indeed believe, not without success. We have also had the opportunity of hearing personally the opinions of every section of its population upon the problems upon which we have been engaged. It is therefore with a certain confidence as to their value that we present our unanimous conclusions for your Majesty's consideration.

It is to be noted, however, that in May, 1915, the Government of the Commonwealth of Australia withdrew its representative from the commission, and the final report does not contain the signature of any representative of that Dominion. No reason is given for this action on the part of Australia.

The main object of the commission was to inquire into, and report upon, (a) the natural resources of the five self-governing Dominions and the best means of developing them; (b) their trade with the United Kingdom, each other, and the rest of the world; and (c) their requirements and those of the United Kingdom in regard to food and raw materials, together with the available sources of supply. Broad as the scope of this inquiry was, it could not be kept within the prescribed limits. The commissioners say:—

During the whole course of our investigations . . . we have been conscious of two strong and impelling impressions.

First, for Empire purposes no survey can be complete without including India, the Crown Colonies, and the Protectorates. In themselves, and even as now developed, they form too vital and important a part of the Empire to be left out of present calculations. But it is plainly evident that their potentialities, measured by any fair standard, are immense,